

# Observations

ON

## TYPHOID OR INTESTINAL FEVER: THE PYTHOGENIC THEORY.

BY

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"So the ordinary face and view of experience is many times satisfied by several theories and philosophies; whereas, to find the real truth, requireth another manner of severity and attention." (*Advancement of Learning.*)

### PART III. (Concluded.)

#### DOES INTESTINAL (TYPHOID) FEVER EVER ARISE SPONTANEOUSLY?

*Summary of Conclusions established in preceding Papers. Their general Effect to show Typhoid Fever to be exclusively contagious. The only Evidence to the contrary Negative Evidence. If Typhoid Fever originate spontaneously, by the same Rule Small-pox and Plants and Animals originate in the same Way. First Origin of all these Things wrapped in impenetrable Obscurity. Small-pox. Itch. Syphilis. Resumé of the whole Argument.*

THERE still remains another mode of dealing with this question, on which a word must be said before I conclude. As all contagious diseases must, in the first instance, have occurred independently of contagion, it is often argued that there is every reason to believe they still do so.

This is the last refuge of the partisan of spontaneous origin.

M. Trousseau, who, although an avowed contagionist, is an equally firm believer in "spontaneity," has put the case very clearly. After observing that the spontaneous origin of contagious disorders is not only a fact that cannot be contested, but is an important element in the development even of the most contagious among them, he adds:—

"In fact, as contagion implies necessarily the presence of two persons, the one giving and the other receiving the morbid germ, it is too obvious that in the first person attacked by a contagious malady the disease must have developed itself spontaneously, and was formed out of its prime elements, under the influence of causes which are completely unknown to us."\*

Following up this view, this distinguished teacher suggests that, many cases in which diseases of this kind spring up in isolated places can only be explained by supposing that the same thing still happens.

Small-pox, that is to say, having once originated *de novo*, must originate *de novo* now. For that, if it is to have any force at all, is what the argument amounts to.

But it is obvious, from what has gone before, that, if this argument applies to the specific contagions,

no reason whatever can be shown why it should not apply to animals and plants also.

Itch, being a contagious disease, two persons are always necessarily implicated in its propagation. But there must have been a time when a first person had the itch without receiving it from another. The itch insect having originated *de novo* once, must therefore originate *de novo* now. The two facts stand on exactly the same level.

The truth is, that the first origin of the germs of specific contagious diseases and of the great procession of organic types which have successively appeared on the earth is alike unknown to us. By the light of science we know no more of the first evolution of the germ of small-pox or syphilis, than we do of the acarus which inhabits the human skin—of the tapeworm which was appointed to live in the human intestine—of the cystic worm which finds a temporary home in the brain of the young sheep—of the miseltoe which fastens on the oak and the apple—or of the different species of mildew which breed in the grain of wheat.\* We know no more, by the light of science, of the first origin of these living types, than we do of the higher organisms on whose substance they feed.

The precise mode in which all these things first came into being is hidden from us by an impenetrable veil.

On the other hand, everything tends to show, that, once created, they all propagate now only in one way. The idea that animals and plants of known species continue to spring up anew is, as we have seen, quite exploded. That small-pox is now only maintained by self-propagation, there are, as we have also seen, the strongest conceivable reasons for believing.

The history of syphilis—and, indeed, of many another contagious disorder—teaches the same lesson. Who among us can say when, or how, or by what process this terrible virus first came into being? Who can name the thrice unhappy man, on whose person, innocent of any unclean contact, it first implanted its foul venom? We all know how it propagates now, and that inoculation by the actual contact of two persons is essential to its continuance.

I have always thought it a very significant fact, that, in this case, where the process of transplantation can only be effected by an act of which two persons, at least, must be cognisant, the idea of spontaneous origin has never once arisen.

Let an isolated case of typhoid fever occur, and, merely because he cannot readily trace the disorder to its source, many a medical man will tell you without the slightest hesitation or difficulty that the disease has, clearly, originated spontaneously. Press him, and he will be ready at once with the argument that there is no reason in the world why the conditions which first gave rise to it, may not again give rise to it now.

But let a young sinner with the peccant mark upon him swear ever so stoutly that the disease he bears has come on spontaneously; let him argue as plausibly and as long as he will against the in-

\* The exact words are these:—"La spontanéité est donc un fait incontestable dans le développement des maladies même les plus contagieuses. Et en effet la contagion impliquant nécessairement la présence de deux individus, l'un donnant, l'autre recevant le germe morbifique, c'est une vérité par trop banale de dire que chez le premier qui fut atteint d'une maladie contagieuse, la maladie se développa spontanément; qu'elle se forma de toutes pièces sous l'influence de causes qui nous sont complètement inconnues." (*Clinique Médicale*, tome Ier, article Contagion.)

\* As a piece of natural history, the whole subject of parasites is a very curious one; and its study, if carried out, would lead to some probably very unexpected inferences. The topic is one which should commend itself, one would fancy, in a peculiar way to the fertile and ingenious mind of Mr. Darwin.

credulity which refuses to see that the conditions which *first* gave rise to this disease, may again give rise to it now, and the very same medical man will laugh him to scorn.

In small-pox and syphilis, where, from the nature of the facts, the evidence bearing on this question is precise and sure, the answer is unequivocal.

The history of these two diseases is at hand to show—as by a great standing experiment—that these specific contagions, so far from being things that spring up at every turn, have, in reality, come into being under conditions hidden in primeval darkness, and of which we have no experience.

The history of many other contagious maladies is precisely to the same effect. The very significant fact of the exclusive appearance of many among them, in the first instance, in particular geographical centres, whence, sometimes only after the lapse of long ages, they have afterwards spread far and wide, leads to the same general conclusion.\*

It will be seen that, in the course of this discussion, I have altogether left out of view the formidable objection to the commonly received theory of the spontaneous origin of contagious fevers, which may be drawn from the incomparably specific conditions shown in my last communication to attach to the reproduction of the specific germs of these fevers even in the living body itself.

In taking a survey of the whole question, the only difficulty, indeed, is to say whether the variety, the massiveness, or the precision of the evidence which may be arrayed against this theory, be the more remarkable.

Stated briefly, the considerations which are opposed to it in the case of intestinal fever, may be thus summed up.

1. The known mode of propagation of this fever is sufficient to explain the actual phenomena, so that the "spontaneous" hypothesis is wholly gratuitous.

2. Under the definite form which it has hitherto taken—that this fever is often actually caused by the products of common putrefaction—this hypothesis, when subjected to a searching examination, entirely breaks down.

3. The evidence which is supposed to render such an hypothesis necessary, would, if of any force, render it equally necessary not only in the case of small-pox, but in that of plants and animals also.

4. In regard to all the specific contagions whose history in past ages is known—such as small-pox and syphilis, for example—there are the strongest possible reasons for believing that, whatever their primary source, they propagate now only by the law of continuous succession.

5. The incomparably specific conditions which attach to the reproduction of the fever poison, even in the living body which is its natural *nidus*, render its evolution by spontaneous origin not only exceedingly improbable, but difficult to conceive of.

To some it will, perhaps, seem that I have laboured

\* This peculiarity is strictly parallel to the primary appearance of particular species of plants and animals, in certain geographical centres, often of very limited area. Mr. Darwin's attempt to explain the fact is well known. I am not competent to say how far it is successful, or whether, and to what extent the same principle is applicable to specific diseases. I may at some future time have something to say on this last point. Meanwhile, the fact itself, as connected with the origin and spread of contagious disorders, would form a very interesting subject for an essay in the hands of any one competent to deal with it.

this point with unnecessary minuteness. A little reflection will show that the fact is not so.

Viewed in relation to this fever alone, the question is plainly one of cardinal importance. So far, however, from being thus limited, the same question recurs in the case of all the other epidemic and contagious disorders.

For some, possibly, a stronger case may be made out; and in these, if such there be, the point at issue must be argued on its own grounds. But on evidence in all respects identical with that which has been here discussed, a claim has been set up for the spontaneous origin of Asiatic cholera, of yellow fever, and of a host of other specific contagions. It was well, therefore, if only to avoid the need of going over the same ground again, to see, once for all, what this evidence is worth.

With the case of small-pox, and that of animals and plants, before us, it will, at any rate, not be required of us to believe in the spontaneous origin of cholera, simply because the succession of cases often cannot be traced.

The conclusion at which we have come is important, also, in another way.

The moment we are entitled to look upon typhoid fever as a disease which, like small-pox, is propagated solely by its own germs, the facts relating to its spread, instead of being interesting only in a negative sense, acquire a value of a *positive* kind.

As illustrations of what is *known to happen*, when a disease so propagated is spread by liquid discharges, they are of the greatest possible help in interpreting the phenomena which characterise the dissemination of other contagions which are spread by liquid discharges too.

By the light of what we have learnt of typhoid fever, the dissemination of Asiatic cholera, of dysentery, of yellow fever, not to mention other members of the group, becomes easy to read.

In addition to all this, a point of high equity is concerned in the argument. On all accounts it is time that the opposite parties in this great debate should be placed in their true respective positions. Whenever, and wherever it is opened, I observe that the contagionists are uniformly treated as if they, and not their opponents, were guilty of dealing in assumptions, and of going against evidence.

I need scarcely remark that the truth is precisely the opposite of this.

At all events, as regards this fever and the other members of the same family group, the contagionist alone proceeds on the solid basis of fact, and has the support of analogy, as massive as it is precise, where from the nature of things the direct evidence of fact is not to be had.

We know that the poisons of small-pox, typhoid fever, and cholera are multiplied in the human body, almost as certainly as we know that corn is raised from its own grain, or that thistles spring from their own seed.

That these poisons are multiplied in any other way is not only pure hypothesis, but, as we have seen, an hypothesis that has, besides, the fatal defect of being entirely gratuitous,—of being in opposition to all analogy that is of any worth, and of being in one of these cases, at least, only tenable at all by heaping one assumption upon another.

P.S.—As I have reason to believe that some rea-

ders have not fully apprehended the drift and purpose of the argument developed in the preceding pages, in consequence of not having seen the series of papers on the same subject, which I published in the *Lancet* in 1856-58-59, it may be well to state that in that series will be found data which, to myself, appear to establish by the most strict and severe induction from incontrovertible facts, the following conclusions.

1. That typhoid or intestinal fever is essentially contagious.

2. That the living body of the infected man is the soil in which the poison breeds and multiplies.

3. That the reproduction of the poison in the infected body, and the disturbance attaching to it, constitute the fever.

4. That this reproduction is the same in kind as that of which we have, in small-pox, ocular demonstration.

5. That the intestinal affection is the specific eruption of the fever, and is related to it in the same way in which the pustules on the skin in small-pox are related to that disease.

6. That the exuviae from the surface which is the seat of this eruption contain, as we should have expected, the most virulent part of the contagious principle.

7. That, as a necessary result, sewers, and the cloacæ which fill the office of sewer, are the principal media for the transmission of the contagion; and, consequently, that in many instances the infected sewer, and not the infected man, appears as if it were the primary source of the specific poison.

8. That, once cast off by the intestine, the contagious matter may communicate the fever to other persons in two principal ways: either by contaminating the drinking water; or, which is by far the more common case, by infecting the air.

In a future paper, I hope to complete the proof, by adducing evidence to show that by treating the intestinal discharges by proper disinfectants, the spread of the fever may be greatly limited, and, in most instances, altogether prevented.

**APOTHECARIES' HALL.** The next Examination in Arts previous to registration as a student under the new regulations will be held on Friday, the 24th, and Saturday, the 25th January. The examination will be partly written and partly *viva voce*. The subjects of examination will be:—1. English history (more particularly of the Tudor period), and the structure and grammar of the English language. 2. Mathematics (viz, the ordinary rules of arithmetic, vulgar and decimal fractions, addition, subtraction, multiplication, and division of algebraical quantities, simple equations, and the first book of Euclid); and natural philosophy. 3. The Latin language (Cæsar, *De Bello Gallico*, Books I and II). Thus far the examination is compulsory; but the candidates may, at their own option, be examined also in (4) the Greek language (Xenophon, *Memorabilia*, and grammatical questions); and, (5) Logic (the introduction, the first book, and to the end of chapter iii in the second book, of Whately's *Elements of Logic*). Proficiency in the whole of the five branches will entitle the candidate to a certificate of special proficiency.

**LEGACIES.** The late Lord Herbert, of Lea, has left £300 to the Salisbury Infirmary. The late Mr. Capel Hanbury Leigh, of Pontypool Park, has bequeathed to the Swansea Infirmary £500; to the Bristol Royal Infirmary £500; and to the Bristol Eye Infirmary £200.

## Lectures

ON THE

### DIAGNOSIS AND TREATMENT OF DISEASES OF WOMEN.

DELIVERED AT ST. MARY'S HOSPITAL MEDICAL SCHOOL.

BY

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#### SUBSTANCES EXPELLED FROM THE GENERATIVE PASSAGES: THEIR NATURE AND DIAGNOSTIC SIGNIFICANCE.

GENTLEMEN,—The class of "data for diagnosis" to which I now direct your attention are derived from the inspection of those bodies, solid or otherwise, submitted or obtained for examination, and reported to have been expelled or removed from the female generative passages. Questions regarding the presence of pregnancy, as to the possibility of impregnation having occurred, as to whether the patient has been recently delivered, etc., may have to be answered; the data for forming a conclusion thereon being those only derivable from inspection of certain substances submitted to us. Uncertain as our diagnosis too frequently is from want of reliable information, it is extremely important when, as in the case before us, we have *substantial* data to go upon, that we should be able to make the most of those data. An accurate conclusion respecting the nature of these bodies is often very necessary; for upon it depend very frequently determinations as to the condition of the patient, and as to the nature of the affection present, of the most momentous character. An erroneous and hasty opinion as to the nature of a particular body submitted for examination may involve loss of reputation to the patient, and no less loss of confidence in the practitioner.

One preliminary caution. An "off-hand" opinion ought never to be given respecting the nature of any substance said to have been expelled from the generative passages. A careful examination of the case should always precede a conclusion as to its nature. It need hardly be observed that an intimate practical knowledge of the normal *anatomy* of the ovum, and a familiarity with its outward appearance, are absolutely essential.

From a variety of circumstances, the substances in question are frequently difficult of recognition; and it is always expedient to place them in water for twenty-four hours, or even longer, at the end of which time they may be in a much more satisfactory state for examination. The importance of adopting this precaution in the examination of cases of suspected abortion it is impossible to over-estimate. What appears on a cursory inspection to be a homogeneous fleshy mass may, after having been soaked for some hours in water, present a most elaborate structure. Great care must be exercised in receiving the statements of patients as to the nature of any particular substance which may have been expelled. Many are desirous of receiving and